SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

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SATURDAY, NOVEMBER 13, 1880.

At the request of Col. W. A. Ross, of England, we publish his open letter to Professor Sorby, who was President of the Chemical Section of the British Association recently, when that body declined to permit a paper, prepared by Col. Ross, to be read.

Col. Ross forwarded this paper to us, and we published it on the 16th ultimo, so that those who desire to judge of the propriety of its rejection, may form their own opinion.

A writer in the last number of the *Chemical News*, of London, a journal well able to appreciate good chemical work, who gives Col. Ross credit "for his interesting and valuable chemical researches," offers in detail an instance of their utility, and acknowledges that Col. Ross's two works, *Manual of Blowpipe Analysis*, and *Pyrology*, are the standard English authorities on this branch of analytical chemistry. It appears to be a strange state of things when such a man must contend against a system of repression and bitter antagonism from those following the same line of investigations, and would seem incomprehensible if similar cases were not continually coming to the surface; thetreatment of Prof. Mohr, and other instances mentioned in Dr. Akin's letter, which recently appeared in "SCIENCE," however, gives a key which solves much of the mystery.

We are not prepared to offer an opinion in regard to the dispute which gave rise to the letter of Col. Ross to Professor Sorby, but the mere fact of a man suggesting "boric or phosphoric acid as a fluid menstruum, instead of borax or microcosmic salt," hardly appears to justify this ostracism from the society of scientists, unless such an innovation is an indictible offense. We have heard of the consequences of speaking disrespectfully of the Equator, but we should have thought that the conduct of a man who insists on using "an aluminium plate" instead of "sticks of messey and obscuring charcoal," would arouse the compassion, rather than the resentment of his fellow chemists, if he be in error.

Seriously, we regret any obstruction to Col. Ross's work; when we consider that the studies which he so ably describes may be conducted with apparatus costing only a few shillings, and that results of the highest order in analytical chemistry may be arrived at, who cannot desire to see encouragement extended to such a practical scientific pursuit? We advise Col. Ross to quietly continue his work, and cease to notice any apparent opposition; if he is ignored by *authority*, let him on his part ignore *authority*, and trust to the sterling merit of his work for its ultimate vindication; his time is surely too valuable to devote to a useless correspondence.

We published, in our issue of the 23rd ult., a paper by Dr. George W. Rachel, claiming for the late Professor Friedrich Mohr, the honor of first publishing the now accepted principle of the *Conservation of Energy*. Like the original article of Professor Mohr on "*The Nature of Heat*," which was at first declined by publishers, this just tribute to his memory, penned by Dr. Rachel, was denied admission to the pages of the scientific monthlies. To-day we publish a later contribution from the same source, in which a biographical sketch of the late Friedrich Mohr is presented to the readers of "SCIENCE" by Dr. Rachel, who has compiled it from original papers placed in his hands for the purpose, by the trustees and family of Mohr. The author has accomplished his task with fidelity and moderation, and the authentic record he presents of a life of utility and self-sacrifice will doubtless be read with interest by our readers. In the *Popular Science Monthly* for July last, a short sketch of the life of Professor Mohr was produced, written by Dr. Fredrick Hoffman, of New York. The essay was brief. We are not aware of the extent of the materials which were at the command of Dr. Hoffman, who, while giving the highest praise to Mohr for his high chemical attainments, made the briefest reference to his claim of making the great discovery of the *Conservation of Energy*, which must forever link his name with physical science.

A meeting of the National Academy of Sciences will be held at Columbia College, New York, commencing on Tuesday, the 16th of November. We trust that the President, Professor William B. Rogers, who is at present sick, may recover sufficiently to preside at the meeting. As yet only seven papers have registered.

ON SOME NEEDED CHANGES AND ADDITIONS TO PHYSICAL NOMENCLATURE.

By Professor A. E. Dolbear.

I.—Physics is now defined to be the science of energy. Previous to 1840 what was known concerning energy was embodied in Newton's Laws of Motion, and was confined to what we may call molar mechanics, to distinguish it from atomic and molecular mechanics, which has since that time been developed. Friction was looked upon as resulting in an absolute loss of energy, and no attempt appears to have been made to find it in other forms. Both Rumford and Davy proved, to their own satisfaction, that friction resulted in the *creation* of heat—an idea entirely different from the conception of heat then in vogue, that it consisted in imponderable corpuscles. No attempt was made to find the quantitative relation between molar energy expended and the heat produced, so that many years elapsed before any advance was made beyond the qualitative work of Rumford and Davy. Even for a time after Faraday's researches had established a quantitative relation between chemical reactions and electricity, the facts were looked upon as rather curious information, out of relation with physics proper, and so the latter was kept strictly what is involved in

$$\text{Energy } E = \frac{m\,v^2}{2},$$

the *form* of the energy being modified by so-called "Mechanical Powers," the lever, the pulley, the inclined plane, etc. Since 1840, however, through the labors of Mayer, Joule, Thomson and others, the quantitative relations between the various known forms of energy have been determined with great precision, and has led to a complete and inclusive generalization of the laws of energy—namely, that the energy in the universe is a constant quantity, the form that it may assume at a given time and place depending solely upon the other forms of energy which are present at the same place at that time. By the *form of energy* is meant *the character of the motion that embodies the energy*, for when there is no motion there is no energy, so that each different form of motion is a different form of energy. Rectilinear motion is a different form from rotatory motion, inasmuch as in rectilinear motion there is a change of position in space of the centre of mass, while rotatory motion does not involve such change, yet both embody energy though each in a special form and each should have a specific name.

Generically, all motion of translation in space is called mechanical motion or molar motion, and its energy, once called its *vis viva*, is proportional to $\frac{m\,v^2}{2}$, and is true for masses of all dimensions. Nevertheless, what a given amount of energy will do depends solely upon its form. Rectilinear motion cannot continuously produce rotatory motion; but vibratory motion can. For convenience in descriptive work as well as for clearness of conception—the latter of great importance—it is necessary to have specific names for the various forms of energy. As each form embodies a particular form of motion, one will only need to specify the various possible forms of motion in order to cover all possible cases. We have then the following table for such mechanical or molar motions:

$$E = \frac{m\,v^2}{2}$$

- Rectilinear, like a locomotive upon a straight track.
- Rotatory, like that of a balance wheel.
- Vibratory, like that of a tuning fork.
- Curvilinear, like that of a projected cannon ball.
- Spiral (unusual), like some forms of projected rockets.
- Vortical, like smoke rings.

As the energy of each of these forms is expressed by the same formula there is no way of identifying either of them, except by some roundabout expression as "The energy of vibration," "The energy of curvilinear motion." It is true that for one of these forms we have a particular name, *sound*, for vibratory motion, provided its frequency is within the limits of hearing, but as the same name is applied to the sensation itself we are without a distinctive name.

II. If instead of large masses we consider atoms and molecules, it will be clear at once that the same *forms* of motions are possible as with the large masses, and the same general descriptive terms are applicable. Thus for an atom there is a rectilinear motion which we call its free path, but for its vibratory motion we use a distinct and and specific name, *heat*. Also for the rotation of the atom in its own plane, we have the specific name, *electricity;* for possible curvilinear spiral or vortical motions there are no names.

The energy embodied in atomic and molecular motions exclusive of rectilinear, that is, that do not involve a change of position of the centre of mass or of inertia of such atom is generally called *internal energy*, and if we let ε represent its value then the complete expression for the energy of the atom will be

$$E' = \varepsilon\,\frac{m\,v^2}{2}.$$

Now such changes and conditions as are involved in what we call *latent heat*, *specific heat* and *specific inductive capacity* are all involved in that factor ε, but the terms specific heat and latent heat are certainly misapplied, for whatever the forms of energy may be, they are certainly not heat, consequently not vibratory. Specific names then are needed for these.

III. The observed transferrence of energy from one atom to another without contact has necessitated the

theory of the ether, and we know the rate of transmission of some forms of energy in this medium to be 186,000 miles per second. It follows then that energy resides in this medium in some form, and it is a matter of experiment to determine the particular form. Thus what is called light and sometimes heat is known to have an undulatory form, and the mechanism of the conditions may be easily perceived. Thus, let the dark

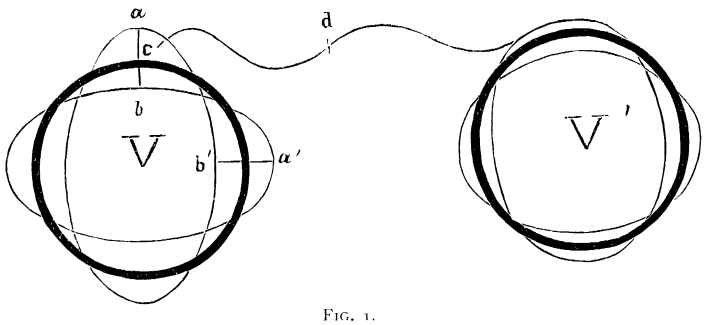

FIG. 1.

ring V (Fig. 1), represent an atom of any matter, say hydrogen (the simplest form of vortex ring). Suppose it to vibrate its fundamental, then will the point *c* move over the line *a b*, and the circle will assume an elliptical form alternating with another ellipse with major axis at right angles to the former, in the line *a' b'*. The line *a b* represents the displacement of the point *c*, in other words, it is the amplitude of vibration of the ring. It is such vibratory motions of atoms that constitutes what we call heat, and we know furthermore that such vibratory motion sets up in the ether surrounding the atom-undulations which constitute what are called rays. Such undulations *c d* travel outwards in every direction, and the length *c d* is called a *wave length* $\lambda = \frac{V}{n}$, *v* being distance traversed during *n* vibrations, *v* being quite independent of the amplitude *a b*. As such displacement *a b*, whether it be small or large, sets up corresponding motions in the ether, it follows that any displacement of matter in ether, whether it be a part of an atom or the whole atom, that is, whether it be so called internal energy or external energy, will originate in the adjacent ether a corresponding movement, which will travel outwards with a velocity which will depend solely upon the translatory property of the ether. This property is sometimes called *elasticity*, but as elasticity is a property of matter and ether is not matter, and as the actual velocity of transmission is so many times greater than in any elastic matter we know, I prefer to say I don't know anything of the specific properties of ether, and do not say that it is even elastic. The undulatory motion in ether is utterly unlike the vibratory motion of the matter that originates it and it ought not to be called by the same name. Furthermore, as atoms differ in mass, so will their rates of vibration differ when

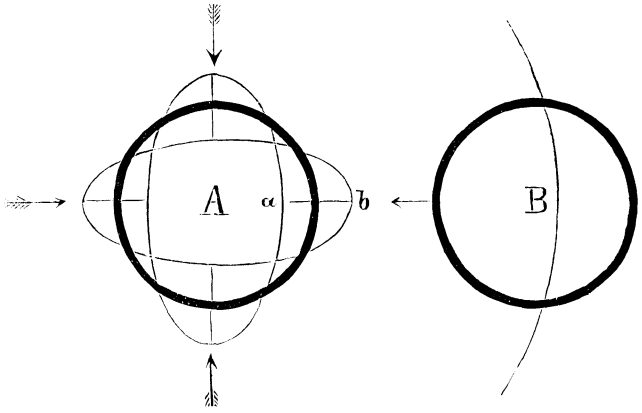

FIG. 2.

they possess the same absolute amount of energy. Velocity in this case will be equal to amplitude *a b*, the space point *c* passes over during one vibration. If *m* and *m'* be two atoms of different masses having equal energy of vibration, then,

$$E = \frac{m\,v^2}{2} = \frac{m^1\,v'^2}{2} \text{ and } \frac{m}{m'} = \frac{v'^2}{v},$$

that is, the square of their velocities is inversely as their masses, so that wave length in the ether will vary as the mass of the atom. As such rays in ether vary only in amplitude and wave length, not in form nor in the medium, it is time to stop speaking of some of them as heat rays, some of them as light rays, and still others as actinic rays. These names characterize *effects*, not the rays themselves; what any one will do depends solely upon what kind of matter it falls upon. What we call light itself is purely a physiological phenomenon and does not exist independent of eyes, and it is hence improper to speak of the velocity of light, however convenient the expression may be. It is what produces light or heat or photographic effects that has velocity and this has the more appropriate name of *radiant energy*.

For a similar reason it is manifestly improper to speak of the temperature of space. Absolute space can have no temperature, for temperature is a function of matter. The temperature that a mass of matter would have in space must depend first upon its own constitution, and second, upon the number and wave lengths of the rays of radiant energy that fall upon it, and these would not necessarily be alike in any two points in space. Let V and V' be two atoms at any distance apart, then if any ray from V falls upon V', the latter will be made to vibrate provided its possible rate of vibration coincides with V, in which case it is a simple example of sympathetic vibration, the amplitude only being less than that of V. If its possible rate is not the same then it will not be vibrated by the ray; in other words it will not be heated by it and consequently it will have no temperature.

IV. Again, consider other physical conditions in and about a vibrating body. Bring any light body that is free to move in proximity to a vibrating tuning fork and such body will be apparently attracted by the prong and will stick to it while the vibrations continue. The average density of the air near the fork is less when it vibrates than when it is still, and consequently any object near it will be more pressed by the air on the opposite side than on the side adjacent to the prong. Precisely similar conditions are present with a vibrating atom. Let A (Fig. 2), be such an atom as before vibrating in its slowest period, *a b* will be the amplitude of vibration, then will there be a less density in the ether at each of the four extremes of the major and minor axes of the ellipses, and consequently a pressure at the four points in the direction of the arrows. The space within which the density is appreciably less may be called the *field* of the atom and if another atom B be wholly or in part within that field it will be subject to pressure towards A. If atom B vibrates synchronously with A there will be no more than a brief temporary disturbance when the two will adhere together by pressure from without and will then constitute what is called a molecule. If, however, the vibratory period of B is not commensurate with A's then after impact the two must separate, either to renew contact and recession or to bound away quite out of each other's field. The same may happen to two similar atoms when the amptitude of vibration becomes very great, they may bound quite out of each other's field, only renewing contact but not cohesion. This is called *dissociation*. This tendency to unite exhibited by atoms and explained as due to purely mechanical conditions was formerly called chemical affinity, but is now called *chemism*. The selective agency observed being due to relative rates of vibration, the possibility of uniting and the strength of the compact depending upon the harmonic relations involved. The motion set up in the ether at the parts of maximum displacement which results in chemism is different from the undulatory and may be distinguished from it as *pulsatory*.

If an atom spins upon an axis at right angles to its plane then any point c, (Fig. 1) in the circumference will be displaced the diameter of the atom every half rotation, and this displacement must set up in the ether a disturbance as great as though the amplitude of vibration had been as great as the diameter of the atom, but the motion of the point c being continuous and uniform instead of vibratory, the motion in the ether must be helical, the diameter of the helix at the atom being just equal to the diameter of the atom, but expanding outwards as a cone and is sometimes treated as a line or tube of force in treatises on electricity and magnetism. This motion in the tube will be right handed or left handed, depending upon which side of the atom the motion is traced from.

Now all the phenomena of magnetism tend to show that wherever it is present there matter is rotating in a plane at right angles to the direction of the magnetic axis, the magnetism being a form of energy in ether, being related to rotating atoms as undulations in ether are to vibrating atoms.

Lastly, there are many good reasons for the belief that matter itself consists of vortex rings of ether in the ether, and that they also embody a certain form of energy, which simply on account of its form is persistent, that is, unlike other forms of energy it is not ex-

changeable with them. In this view inertia is a law of energy and not a property of matter.

The following table gives a synoptical view of the various forms of energy and the names they have. Where there are no names an interrogation point is placed to indicate the lack. To the writer it appears as if each specific form of energy should have a specific name, but he is aware of the difficulty of finding suitable names and getting them adopted. If this want is felt by others then a committee of suitable persons might be appointed by the American Association for the Advancement of Science, who might consider and recommend appropriate names as did the British Association for Electrical Science some years ago.

TABLE OF FORMS OF ENERGY.

1. *Mechanical or Molar Motion.*

$E = \frac{mv^2}{2}$

Form.	*Name.*
Rectilinear	?
Rotary	?
Vibratory	Sound.
Curvilinear	?
Spiral	?
Vortical	?

2. *Atomic and Molecular.*

$E' = \varepsilon \frac{mv^2}{2}$

Rectilinear	Free path.
Rotatory	Electricity.
Vibratory	Heat.
Curvilinear	?
Spiral	?
Vortical	?

3. *Atomic and Molecular.*

$E' - E = \varepsilon$

?	Specific heat.
?	Latent heat.
?	Specific Induc. Capac.

4. *In Ether.*

$E = ?$

Rectilinear			?
Vibratory	Pulsatory		Chemism.
	Undulatory		Radient Energy.
		Pseudo	Light.
			Heat.
			Actinism.
Rotatory, Curvilinear,	Circularly and Elliptically		Polarized Light.
Spiral			Magnetism.
Vortical			Matter.

THE SPANISH MACKEREL AND ITS ARTIFICIAL PROPAGATION.

BY CHAS. W. SMILEY.

This fish, *Cybium Maculatum*, is in general appearance very like the common mackerel. It is larger, however, averaging seventeen to twenty inches in length. When first described it did not exist in our waters, but was abundant in the Gulf of Mexico and the Caribbean Sea. Its first appearance was about 1850. It then began to be taken as a food fish. It began to be caught in the Chesapeake about 1870. About 1872 or 1873 it appeared in Narragansett Bay, when three or four hundred were taken at one haul of the seine, but the fish did not subsequently reappear.

The Chesapeake Bay has been annually visited by large schools for several years, where it is known as the "Bay mackerel." None were known to have been marketed there prior to 1870, but in 1879 1,300,000 of this fish were sold, and the season of 1880 is expected to yield 2,000,000. They were taken in pound nets and gill-nets. At Cherrystone, Md., there are fourteen pounds, which average a catch of 500 to a day. As many as 4,000 per day have been taken in a single pound on the eastern shore of the Chesapeake, while 2,500 is not a rare catch with one pound. The Bay fish are, however, smaller and leaner than those caught further north.

As this fish refuses the hook its capture is limited to pounds and nets. The first pound in the Bay was built in 1875. Now there are 164. The first gill-net used there was in 1877, while now there are 175 men fishing by this means. A net 100 fathoms long will average forty fish per twenty-four hours, the fish weighing from one and a half to two pounds each.

In the New York market the price per pound ranged from eighteen to thirty cents during 1879; for May, 1880, from fifteen to forty cents; but owing to the large shipments in June the price fell to ten to fifteen cents. On special occasions the fish have been sold readily at one dollar per pound. The catch of 1873 at Newport, R. I., was sold at prices varying from seventy-five cents to one dollar per pound.

This fish usually appears in the Chesapeake in May, when the temperature has reached 65° or 70°, and the number increases until the middle of June. They remain abundant until September, and diminish as the temperature of the water falls, until, in early October, nearly all have disappeared. They come in small schools, but later get scattered, and often quite isolated. Before leaving, the schools seem to be somewhat reformed.

The United States Fish Commission, under the management of Professor S. F. Baird, the Secretary of the Smithsonian Institution, has long desired to experiment upon the artificial propagation of this fish, but has been deterred by the lack of knowledge of its spawning time and places. These were both discovered June 1st by Messrs. Earll and McDonald, Assistants of the Commissioner. At that date the lower Chesapeake, especially Mobjack Bay, was found to contain large numbers of spawning mackerel. This opened the way for experiments, and Professor Baird was ready to seize upon the opportunity. He directed Mr. Earll to make every effort to hatch some fish.

June 21 Mr. Earll started for Crisfield, Md., on the eastern shore of the Chesapeake, and during the following ten days there conducted his experiments.

He found the number of eggs produced by a single fish to be from 50,000 to 500,000, according to the size of the fish, the latter number having been taken from a fish weighing one and three-fourths pounds. Instead of all the eggs ripening at once, as is true in the case of the shad, only a part are thrown at a time, and at intervals of a few days, probably extending through two or three months. This is analogous to the cod, which deposits its eggs at intervals during five or six months. Different individuals of mackerel were found to vary in their time of spawning; some ripening a considerable time before others, and the males seeming to ripen somewhat in advance of the females. From 40,000 to 130,000 eggs were obtained at one time from a single fish. The shad, however, yields only 20,000 to 30,000 as its fruits of an entire season. The cod, on the other hand, are so prolific that a twenty-one pound fish has yielded 2,700,000 eggs, and a seventy-pound fish has yielded 9,000,000 eggs.

When the fish had remained in the nets several days Mr. Earll found that the most of the spawning females had deposited all their ripe eggs. The greatest quantities were secured from individuals that had remained in the pound but a few hours. It is believed that when confined the female presses against the netting in its efforts to escape and produces an abnormal discharge of eggs; but it would result in the impregnation of a much larger number of eggs than would chance to be fertilized in a natural way. The males and females being caught side by side in considerable numbers, both eggs and milt would be present in the water in such quantities that

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they could not fail to come in contact before vitality is lost. A half hour after contact with the milt, the eggs swell and become too hard to be broken by pressure of the thumb and finger. Their specific gravity is now so nearly equal to that of salt water that when the water is at rest they float upon its surface, remain suspended in the water, or occasionally sink slowly to the bottom. The least current will cause them to be distributed through the liquid. Mr. Earll discovered a small oil globule in each egg which serves the purpose of buoying it. The impregnated egg is also so transparent that the fishermen, who are not usually very observing, would never suspect their presence. The eggs are smaller than eggs of almost any other species, and have an average diameter of only one-twenty-eighth of an inch. It has been estimated, it will be seen, that 21,952 would make a cubic inch, and a quart of 57¾ cubic inches would hold 1,267,728 eggs.

The period of hatching is greatly influenced by the temperature of the water. The average temperature during the experiments at Crisfield was 84° Fahrenheit. Ten hours after contact with the milt the outline of the fish could be discerned by the naked eye. The fish is formed with the curve of the back at the lowest point of the egg. In fifteen and one-half hours the fish began hatching. In eighteen hours one-half of the eggs had hatched, and in twenty hours all were out. Experiments in water at 78° Fahrenheit showed that twenty-four hours were necessary for hatching. A more remarkable effect of temperature is observable in the case of the cod. In water at 45° cod have been hatched in thirteen days, but in water at 31° fifty days were occupied in hatching.

The newly-hatched mackerel are about one-eighth of an inch in length, and so small as to escape through wire cloth with thirty-two threads to the inch, and are almost colorless. The food sac, situated well forward, is quite large in proportion to the body, the anterior margin extending to the lower jaw. While floating on its back for several hours, during its helpless condition, it passes safely over the heads of its enemies, and is protected from being wrecked in sand or weeds. After a few hours, becoming more vigorous, it gets to a depth of an inch or more below the surface of the water. After a day or two the food sac is less prominent, and the fish experiences less difficulty in swimming at various depths. The young mackerel hatched by Mr. Earll were so hardy that forty were confined in a goblet without change of water for two days before the first fish died; others placed in water which was allowed to cool gradually and immediately transferred to water ten degrees warmer, were not injured in the least. In fresh water they slowly sank and died in a few hours. Mr. Earll also found that a fair percentage of eggs could be hatched in still water with but one or two changes during their development. Eggs taken at 6 P.M., and allowed to remain in a basin of water till morning, when another change was made, hatched with very small percentage of loss. Samples of all the different stages of development were preserved in alcohol and glycerine for the National Museum. Over half a million were hatched by the various methods and at various times.

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In spite of the loss of time, occasioned by his continued ill-health, his sharp, grasping intellect enabled him to pass with honors the examination for admission to university study (*Abiturienten-Examen*) at the comparatively early age of seventeen (1823). After five years of further practical work as an apprentice and clerk in his father's store and pharmaceutical labratory, he went to *Heidelberg* (1828) studying chemistry with GMELIN. After another lustrum spent in various studies, there as well as at *Berlin*, and finally at *Bonn*, he returned to his home at *Koblenz* (1833) with the degree of *Ph. D.** After having passed his *State's Examination*, he married, and had, at the time of his death, two daughters and two sons. The faithful companion of his life, an excellent wife and mother, also survives him.

In 1840 his father died and he then took charge of the inheritance faithfully, and for seventeen years conducted the *Mohren-Apotheke*, as the establishment was popularly called. In 1857, however, he disposed of it in order to devote, in retirement, his entire energies to scientific research. He did so for a period of about six years, and then had the misfortune to become involved in pecuniary difficulties arising from the failure of a manufacturing establishment belonging to his son-in-law, in which he had been special partner. The honest fulfillment of all his engagements cost him nearly all his fortune, and was the direct cause of his removing to *Bonn*, where he settled in 1864 as lecturer (*Privatdozent*). The chair of Pharmacy becoming vacant a year and a half after his settlement at *Bonn University*, he was appointed to the place. He owed this appointment to the direct influence of *Emperor* then *King* WILLIAM, and the *Empress* then *Queen* AUGUSTA. The latter always took a lively interest in his welfare, which dated from their long residence at *Koblenz Castle* before the Prince's accession to the throne. The Princess had always been fond of the great man's company, his conversational powers and his manner of reading being of an unusually high degree of perfection.** In a letter to the writer, his accomplished daughter, *Miss* ANNA MOHR, who acted as his amanuensis for many years, states:

"In our family circle where he felt himself surrounded by loving care, I have never seen him otherwise than happy and contented. Full of feeling and sparkling with humor, he always was appreciative of everything that is noble and beautiful in art and nature. Music and poetry were always especial favorites with him and while BEETHOVEN, MOZART, HAYDN and WEBER were his ideals in the former, GOETHE, SCHILLER, SHAKSPEARE, HOMER, etc., were to him as old acquaintances. His wonderful memory enabled him to recite for hours SCHILLER'S Ballads or his *William Tell;* GOETHE'S *Faust;* HOMER'S *Iliad*, and many, many other works of those and other poets. And not only was his recitation masterly and perfect, his reading power of serious, as well as of comical pieces was unsurpassed. . . . . He would at the same time master any dialect, new to him, in a few hours, and his many friends and acquaintances owe him many hours of bliss and happiness and many a pleasant evening."

After having thus fonnd a congenial sphere of action, his genius—no longer dragged down by pecuniary cares—attained full sway. In quick succession he published that series of, not very numerous, yet very important, works which will make his name immortal. His lectures also, those at the University as well as many others which he delivered in clubs and societies at *Bonn, Cologne, Koblenz, Krefeld* and other neighboring cities and his many contributions to scientific as well as other magazines and periodicals won him the hearts equally of his students and his lay-hearers. Of this the immense throng of people, belonging to all classes of society, that attended his funeral, was a sure indication.

But, in spite of all this popular recognition, he was not allowed to take that commanding position to which he was entitled by the superiority of his genius. We need only remind the reader of DR. AKIN'S letter published in No. 15 of "SCIENCE," to suggest the causes for the otherwise almost incomprehensible fact that MOHR remained an "*Extra-Ordinarius*" up to the time of his death. The reason is obvious. Even the Hohenzollern did not undertake serious intervention in his interest in regard to this matter; for, although his loyalty and patriotism were proverbial, his radical views in regard to things theological which he always fearlessly confessed, and his unflinching attacks on erroneous views in science, regardless of what position those who proclaimed them might chance to hold, were sufficient causes for the failure of the powers that be to promote his attempts. He remained undisturbed because he recognized *authority* in matters political, but he was not promoted, because he did not feel bound by any *authority* in theological and scientific matters, unconditionally.

The *Emperor* desisted from interfering after experiencing a resistance on the part of "*Cultusminister* FALK" against MOHR'S promotion, which he could not overcome.

When MOHR settled at *Bonn University* as a *Privat-Dozent*, he was 57 years of age, *i. e.*, older by several decades than the average of his colleagues, being the senior of most members of the regular faculty themselves. But more than this, he had already at that time shown his inclination and his ability to reform, nay, to revolutionize some of the many branches of science which he mastered (theoretically and practically). This was more than mediocrity and even famous men are willing to endure. And to just such influences MOHR himself—who knew all the various intrigues against and reports about him, which he never raised his finger publicly to lay bare or refute—attributed the bad treatment which he received.

His eldest son, MR. CARL MOHR, an able chemist, and an accomplished contributor to scientific magazines, writes feelingly, about this matter, as follows:

"....yet it would be interesting to expose without fear or favor the dark doings of that '*official science*' of such men as ——, ——, ——,* etc. These men do not want to recognize anybody as their equal who does not sail under their colors; followers and panegyrics are all they care to be surrounded with. But they hate and fear men of an independent turn of mind who dare have convictions of their own and dare express them, regardless of consequences to either themselves or others. Father has, for instance, by his sharp and telling hits of criticism in his Commentary to the '*Pharm. Bor.*' made enemies of the whole official clique of ——, ——, ——, and others at *Berlin*. Those men, instead being thankful to a man, far superior to them, who has pointed out errors, and shown how to correct them, have persecuted him to their hearts' content. When, therefore, the commission for the preparation of the '*Pharmacopœa Germanica*' was to be appointed, he was excluded from the list of commissioners intentionally and ostentatiously. The man who really was the *Nestor of Pharmaceutics in Germany*, author of such unrivalled standard works as his '*Pharmcopœa universalis*,' the *Commentary*, etc., above referred to, a 'Manual of Pharmaceutical Practice,'** a 'Textbook on the Art of Dispensing,'*** and others, was ignored insuch a disgraceful manner. It was a shameful performance, one that has no equal in the whole history of Science."

The narrative of these occurrences is one of the best illustrations of DR. AKIN'S views, as expressed in his letter to PROF. STOKES, alluded to before.

But, although the illustrious man was thus slightingly treated by men, generally far inferior, none of them superior to him, principally on account of his superiority and of the fact that most of his views and arguments were un-

* The honorary title of *M. D.* was conferred upon him in later years; he also received the title of *Medizinalrath*, and was for a period of over thirty years the pharmaceutical adviser and member of the *Rhenish Medical Council*. He furthermore was elected corresponding or honorary member by several academies, numerous pharmaceutical and scientific associations in general, among the former being the *American Pharmaceutical Association* and the *Philadelphia College of Pharmacy*.

** He at this time delivered a course of lectures to *Princess* AUGUSTA at his house, comprising experimental chemistry and applied mechanics (models of steam-engines, etc., being prepared for this special purpose), the *Prince* and his eldest son (now the *Crown Prince of Germany*) attending when they stopped at the Castle.

* The list of names—I am sorry to say—embraces some of the most renowned professors at *Bonn* and at *Berlin*. MOHR'S intention was, as I am informed, to give a detailed account of the various intrigues against him in a work he was about to publish, when death overtook him; to accuse his persecutors and enemies, and lay at their door the guilt, of having deprived him of due recognition and promotion to the place and honors of an '*Ordinarius*,' and to justify before the public his conduct of not having until then stooped to answer and refute the indignities thus heaped upon him. G. W. R.

** *Lehrbuch der Pharmaceutischen Technik;* published in *six* editions (first, 1846) trveral times translated into French and English.

*** Rezeptinkunst.

answerable,* he was highly esteemed by the independent members of the profession at large in Germany. The last meeting of the '*German Apothecaries' Association*' which he attended, reading the inaugural address, was made the occasion of quiet and impressive ovations which the modest man received with deep feeling. The kind and respectful regard with which the venerable scientist was treated by almost all the members present, was often referred to by him in his family-circle with pride and satisfaction during the twelve months which were still allotted to him. He felt at that day, if ever, that he had not lived in vain, and that the seed which he had sown would not be lost, but that coming generations would yet profit from and be benefited by it.

Besides the works mentioned, he wrote the following: *The Mechanical Theory of Chemical Affinity, etc.*;[1] *General Theory of Motion and Force as a basis of Physics and Chemistry*:[2] *Manual of volumetric Analysis*;[3] edited five times, and last, but not least, a *History of the Earth; Geology founded on a new basis*,[4] which was edited twice.

Of the various improved analytical methods devised by him may be mentioned the proposal to use oxalic instead of sulphuric acid (GAY-LUSSAC), to determine the relative proportion of alkalies and acids contained in a salt; his combination: Sodic Carbonate against Iodine-solution; or, better still, Sodic Hyposulphite against Iodine-solution, and his beautiful determination of Chlorine, by the use of Argentic-Nitrate-Solution, with Potassium Chromate as indicator; of the many instruments invented by him, the self-acting stirring apparatus, with clock-work arrangement, a pill-machine, an apparatus for preparing infusions by the use of steam, another for extraction by means of ethers, and his improved burette, with compressing faucet. His *Manual of Volumetric Analysis* in which these devices and many others of like importance are described, is considered one of the first standard works in the domain of analytical chemistry, and has been translated into various languages.

The attempt, first made by him in his *Mechanical Theory of Chemical Affinity*, to promulgate the theory that chemical affinity is a mode of motion, inherent in matter, and is measurable only in so far as we can measure the heat that is liberated and bound up during the union or separation of two elements, is one of his greatest efforts. LIEBIG† himself always valued this work very highly, and it is certainly one of the most brilliant manifestations of MOHR'S genius, as will be seen from the following extracts:

"The union of two bodies by combustion always liberates a certain portion of this motion which appears in the shape of heat. Another portion remains in the product of combustion. We are only able to measure the former, not the latter, and even of the former we are unable to say how large an amount is due to one body and how much to the other. If one gramme of hydrogen unites with eight of oxygen, 34,462 units of heat are liberated. These indicate the amount of motion which both gases contained when yet ununited, as compared with the water resulting from the union. In the latter itself there is yet a certain amount of motion, as its liquidity and its proneness to vaporization successfully prove. From the oxygen contained in the water we are able to liberate yet another amount of heat by uniting it with potassium or with zinc, because potassa and zinc oxide are more apyrous than either potassium or zinc. Now, it is impossible to know what portion of the 34,462 units of heat comes from the hydrogen, and which from the oxygen, and furthermore, what amount there is yet in the water. Therefore, we cannot reduce to an absolute measure this property of chemical affinity, only the portion that is liberated in the shape of h at.

"This example also shows how enormously more efficient the motion that bodies contain as chemical affin'ty, is than that which they contain as heat. The water produced (9 grammes) contains 9 u. of h., while the mixture of both gases before union, contained only 2¼ u. of h. (the specific heat of hydro-oxygen-gas being 0 25). But, since there was a development of 34,462 u. of h. during their union, by combustion, it follows that the motion existing in the mixture as chemical affinity, is 15,316 times that contained in it as heat."

Again:

"If iron develops heat while oxidizing, the dense condition of the oxygen in the resulting ferric oxide is certainly an effect due to chemical affinity, but the potential energy of the oxygen is no longer found in the oxide; it has been separated. The heat liberated during the combustion of the iron in oxygen-gas is the surplus of motion which iron and oxygen contain more than Ferric Oxide."

And again:

"If carbon and oxygen unite to form carbonic acid, there is no change of volume, and so it is with a mixture of chlorine and hydrogen. Their specific gravity after chemical union is the same as before such union took place, because their volume remains unchanged; yet a great amount of heat has been liberated.

"Thus it is not true, as has been formerly assumed, that we may compare—for the purpose of measurement—chemical affinity to mechanical force by calculating the amount of force necessary to compress a mixture of gases so as to give it the density possessed by the product resulting after chemical union has taken place. (Hydro-oxygen-gas against water."

The quintessence of this unique volume is contained in these two theses:

1. Loss of chemical affinity or liberation of heat always means: *Higher specific gravity, higher melting point, higher boiling point, insolubility, chemical indifference, rigidity and development* of but *little heat* on combustion.

2. Increase of chemical affinity and absorption of heat always mean: *Lower specific gravity, lower melting point, increasing solubility, proneness to chemical combination, softness, development of much heat* on combustion.

Mr. CARL MOHR, in a biographical sketch of his father's life and works, says of them:*

"These two axioms comprise almost the whole range of chemical processes, and they are a *mechanical theory of chemical affinity*, in the very same sense that we have a *mechanical theory of heat.*

"As an example to illustrate the first thesis, the reader needs to be reminded only of the chemical relation that exists between acids and bases: sulphuric acid against alkalies, such as caustic potassa or quick-lime. The process of saturation is accompanied by the liberation of considerable heat; the products have a very high melting point and are chemically indifferent.

"To illustrate the second thesis, a good example is the formation of carbon sulphide. As is well known, this process requires a considerable expenditure of heat, and the product thus obtained is volatile at a temperature far below the degree of temperature required for its formation. This heat, taken up by the carbon sulphide, is contained in it as chemical affinity, and is evident by its low specific gravity, its low boiling point, its proneness to decomposition, and the increased development of heat during the combustion of its elements."

The great principle underlying all this reasoning is that of "*the conservation of energy and the correlation of forces*," of which, as we have shown, he was the first exponent.†

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[1] Mechanische Theorie der chem. Affinität; Braunschweig, Fr. Vieweg & Sohn; 1868.

[2] Allgemeine Theorie der Bewegung und Kraft, als Grundlage der Physik und Chemie; Braunschweig, Fr. Vieweg & Sohn; 1869.

[3] Lehrbuch der chemisch-analytischen Titrirmethode; Braunschweig, Fr. Vieweg & Sohn; 1855 (1877).

[4] Geschichte der Erde; eine Geologie auf neuer Grundlage; Bonn, M. Cohen & Sohn; 1866 (1875).

* Many of the suggestions contained in his earlier works were made use of in the '*Pharm. Germ.*' by the very men who were his life-long enemies, because these and other views were freely and sometimes sharply expressed by MOHR. Many of his original ideas on other subjects than *Pharmaceutics*, especially those on *Geology*, were also confirmed by later evidence; but this equally did not serve to reconcile his opponents in this line of research which comprised nearly all '*official geology*' in Germany.

† At the eve of its publication, the great chemist wrote to MOHR as follows:

"I am impatient to see your new book, for you seem to have treated in it of nearly everything that we need to know something definite about, in order to make chemistry a real science; nobody as yet has really had a clear conception of affinity; we simply stuck to facts, that was all. It has been just so with the melting-point, the gaseous condition, the boiling-point, etc."

* *Archiv der Pharmacie;* Vol. 216, 1880.

† See the paper published in No. 17 of "SCIENCE."

The work, as may be gleaned from the few quotations given above, abounds in a variety of new and original ideas, many of them elaborated, others rather fragmentary, but all of them bearing the stamp of the author's genius and containing an inexhaustible source of information and elucidation on the somewhat abstract subject of chemical theory. It may be added that MOHR does not approve of the modern valuation of atoms, but uses exclusively the old formulæ.

The *Allgemeine Theorie, etc.*, is written in the same vein, in fact it is introduced by the author as a supplement to the former, the 'Mech. Theory of Chem. Aff.' Its purport is to lay out the different regions of physics as far as they relate to motion and force; it also gives an outline of the method by which the correlation of forces and especially its grandest practical achievement, the 'Mechanical Theory of Heat,' should be made the basis of natural philosophy.

MOHR insists particularly on the complete eradication of the wrong use of the two words: motion and force: "Motion (*actual energy* of the English writers) implies a change of place, and consequently force (*potential energy*) comprises those states in which a change of place does not obtain." (l. c. p. 12).

"Steam force is therefore a correct expression and designates the tension (potential energy) of the steam. As soon as the piston moves, the tension disappears (potential is converted into actual energy). In the flying wheel and the balancier we have motion (actual energy) and *not* force (potential energy) when 'running empty,' but motion *and* force, when there is some work done at the same time" (Ibid.)

The translation of these few passages will suffice to show the principal aim of the author: To obtain perfect clearness of expression by distinguishing between the two forms, to either of which every '*causa efficiens*' in nature belongs, viz., force (potential energy) and motion (actual energy). Prompted by the same desire, he makes these divisions in classifying those '*causæ efficientes*,' to wit:

"A. MOTIONS.

1. *Motion of bodies* or *progressive motion.*
2. *Light* and *Heat* in the state of radiation or *radiating motion.*
3. *Common, conducted heat* or *conducted motion.*
4. *Galvanism* or *flowing motion.*
5. *Chemical Affinity* or *clinging inherent motion.*

B. FORCES.

1. *Gravity.*
2. *Magnetism.*
3. *Electricity.* (*Static.*)
4. *Cohesion.*"

This division is based on the property of introconversion which is peculiar to the former, not to the latter. The five 'motions' are introconvertible and also convertible into either of the latter (the forces), while these are only convertible into the former.

The reason for his peculiar view that *heat does exist in two different forms of motion*, he states in these words:

"After due consideration of the matter in question, I feel compelled to separate *radiating* from *conducted heat* Radiating heat is not really heat: it does not expand bodies; it does not act on the thermometer nor on the thermo-electric column. That is what conducted heat does. The fact that radiating heat is converted into conducted heat, as soon as it strikes a body which does not reflect it, is no reason why we should declare the heat-rays to be heat already, for the luminous rays and the galvanic current must undergo the same change (before being capable of acting on the thermometer or on the thermo-electric column.) The mode of motion obtaining in the heat-*ray* differs so radically from that in *conducted* heat, that the separation adopted above seems to me fully justified. The circumstance that the heat-ray does not penetrate the different fluids of our eyeball, or if it do penetrate them, is not transmitted through the optic nerve, constitutes only a mechanical difference as compared with the visible ray of light, and we justly conclude that this depends entirely on their different wavelength."

The manner in which the various modes of motion and of force are measured by means of the application to them of the mechanical theory of heat, is particularly dwelled upon. From this it appears that of the *five* modes of motion, *two* only—mechanical motion (of bodies) and heat are reducible to absolute measurement. These two may be compared by means of the unit of heat, expressed in kilogrammometers.

Another line of thought which MOHR has first dared to follow and to pronounce upon, is the one dealing with the real value of the use of mathematical expressions in natural philosophy.

He holds that the value of mathematics is only secondary, and is, as a rule, greatly overrated. He says:

. . . "For, such propositions are proved by logical reasoning only, not by applied mathematics. A formula is nothing but the mathematical expression of the true inwardness of certain phenomena, as it has been previously found by intellectual observations and reasonings. What is obtained as a result through the formula, is in it from the beginning; it is not at all a discovery made by the mathematic an. If the first equation is wrong, the conclusions arrived at will be wrong. The ancient mathematicians who knew nothing of algebra, logarithms and the differential calculus, had to reason logically exclusively; even the lack of the Arabic numerals was a great drawback to them. That we are able, by the help of all these advantages, to deduce quicker and more accurately a number of relations from a given equation, only diminishes the intellectual merit of our work but not the practical value of the result. Mathematics have only one object, to wit: to evolve from certain given conditions the unknown quantity."

It cannot be denied that there has been, of late, an obvious tendency to overestimate the value of mathematics in the philosophical investigation of physical problems. And it is not the least of MOHR'S many merits that he has conclusively shown that their importance is, secondary only, and that their intrinsic value is far below that of logical deductions. "A mathematician can never discover a new law in physics; that is done by observations only and by the logical reasonings, based on such observations."

By thus defining the proper limits and the legitimate domain which mathematics should occupy in natural philosophy, MOHR has certainly rendered an invaluable service to numerous students of nature. Great numbers of such have been frightened by the many hundreds of pages filled with mathematical formulæ, with which we now see treatises on physics so profusely interspersed.

A perusal of this somewhat fragmentary work does not fail to leave the impression in the mind of the reader what a great misfortune it was that the author should have been called away before he had given us a more complete and comprehensive treatise on the subject; numerous requests addressed to him with this view were invariably met, as we are informed, by a modest decline, on the ground that much was to be done before this could be ventured upon.

His immense capacity for progressive thought is best illustrated by quoting from one of the "*Appendices*," happy after-thoughts, to this work. There he says:

"In determining the conception of the word *force* in contradistinction to *motion*: gravity, cohesion and magnetism seem to be without any inherent motion, as we have no indication that there is such motion. On the contrary, the tension of compressed air, the action of an explosive mixture, etc,, appear in the form of inherent molecular motion, which could appear dead only in comparing it to mechanical motion (of solid bodies). In a compressed gas there is perceptible heat and chemical affinity (motion). The perceptible heat acts on the thermometer only, but not as a force on the walls (of the enclosing vessel), while that amount of heat which is available for expansion, and which, while the (movable) wall of the enclosure (piston in cylinder of steam engine) is receding, is actually used for expansion, implies tension directed outwardly. Since, therefore, we recognize in this the effects of actual energy (*motion*) as being of the same nature as those of potential energy (*force*—gravity, for instance), acting by pressure or tension (supported or suspended weight), the question arises *whether magnetism, gravity and cohesion are not different forms of an inherent molecular motion of which we have, as yet, not the slightest conception.*"

This idea has occupied his mind during the last years of his life particularly, and he has published one of a series

of articles* on the subject of cohesion and the atomic theory, the latter of which he attacks with all the acuteness we are wont to find in his writings.

In regard to MOHR'S *Gesehichte der Erde*, which he rightly calls: *Geology on a new basis*, it is impossible to say little, if it should be spoken of at all. Suffice it, however, to state that it is not what is usually called *Neptunism* what MOHR advocates; it is free from any kind of "ism," as the man who wrote it himself was. But in the generally accepted plutonistic geology, fire plays such an omnipotent *rôle*, that any deviating view, any appreciation of the true state of things geological, is believed to deserve that name from the outset. The so-called Neptunism of MOHR has, however, nothing in common with the impossibilities of the Old Neptunistic School of WERNER and his followers.

There is this radical difference between the views, as yet generally accepted, and those advanced by MOHR:

His aim is to show *in what manner* the several minerals and rocks, composing the outermost, accessible strata of our earth, have been formed; the *question of time* he considers to be not only irrelevant, but also very difficult of ascertaining. His firm conviction of the falsity of the nebular hypothesis and the previous fluid state of the earth and the present molten state of its interior has been strongly supported of late, not only by other *savants*, but by a number of facts and reasonings. The foremost among the former is embodied in the results of careful measurements of temperature in the deep boring (4052') at *Speremberg*, near *Berlin*, conducted by the most learned and experienced mining officials of the Prussian Government. They show that the old supposition of 1° C. increase of temperature to each 100' is fallacious, and that this increase diminishes constantly in going down. Here is the table:

Depth.	Temperature.	Increase per 100 ft.	
700 Rh. ft.	15 654° Réaumur	1.097° R.	2.468° F.
900	17 849	1.047	2.356
1100	19 943	0.997	2.243
1300	21 937	0.946	2.128
1500	23.830	0.896	2 018
1700	25 623	0.846	1.904
1900	27 315	0.795	1.789
2100	28 906	0.608	1.368
3390	36 756		

While for the lowest 100 feet it was: 0.445° R. (1.001° F.)

"This would make the final result, as stated by MOHR," says Dr. KLEIN,* "very well founded, nay, indisputable, viz.: 'that the increase of temperature *will cease altogether*;' not at a depth of 5000' or 6000', as MOHR will have it, but, at all events, at a depth considerably below 100,000'."

Thus, in addition to the many astronomical, physical and chemical reasons, which are irreconcilable with the nebular hypothesis and with the theory of the molten interior of the earth, derived from this hypothesis, MOHR has pointed out an irrefutable fact which supports this position.** For, continuing the slight increase found in a proportionate ratio, at the point where there will be no more increase, the temperature actually obtaining will fall much below the melting point of lead; this had been predicted by MOHR on other grounds, before the measurements made at *Speremberg* had been published.

Chemistry, especially, is ingeniously applied to geology in MOHR'S work, and from the chemical constitution and physical properties of the various minerals and the rock-formations which they compose, conclusions are arrived at which throw a new light, in many cases, on the probable origin and subsequent metamorphosis of the various component parts of the surface of our globe. To mention only one important result, we may refer to the different properties of the Silicate rocks having volcanic origin, as compared with those having, until lately, been supposed to have a 'plutonic' origin. The several properties which the one kind possesses are not found to be properties of the other, and MOHR, therefore, takes strong ground to deny their fiery origin.

Among these properties, demonstrating their crystalline origin, may be mentioned the following:

1. That Feldspar, Augite, Hornblende, Mica, Rock-crystal, Quartz, etc., as well as the rocks which are composed of these minerals, all show minute cavities containing water which are not found in rocks that have undoubtedly been molten, such as Obsidian and other lavas.

2. That the specific gravity of the silicic acid contained in them is greater (2, 5—2, 6) than that in volcanic rocks (2, 5—2, 3).

3. That by melting these crystalline rocks (Basalt, Granite, etc.,) their specific gravity is reduced in the same proportion.

4. That petrified wood, fossils, and other pseudomorphoses of organic origin, and even organic matter unchanged (Asphalte in Granite), is found to be enclosed in them.

5. That the crystalline rocks decay much easier and quicker than the volcanic molten rocks.

6. That many chemical actions, combinations, etc., would be impossible if a molten condition was presupposed, etc.

These and other properties of less importance undoubtedly form a strong array of proofs against the '*plutonic*' or fiery origin of the Silicate Rocks.

The formation of lime-deposits takes place according to MOHR in the ocean by the following bio-chemical processes:

The sulphate of lime contained in the sea-water is assimilated by the marine plants (*Algæ* and *Fucus* especially), and by them decomposed in the course of their vital processes. While the sulphur enters into albuminous compounds, the lime unites with carbonic acid, and both go to form part of the plant itself. The plants serve as nourishment to the myriads of minute animals (*Rhizopoda* and *Foraminifera*), which populate the oceans, and while the carbonate of lime serves to build up their shell, the sulphur is eliminated by their bio-chemical process as sulphurous and hydrosulphuric acid. These shells which lie from 10 to 15 feet deep at the bottom of the oceans, are the chalk and lime-beds of the future.

The presence of organic matter in meteoritic masses and the absence of carbon in meteoritic iron are pointed out by MOHR as further proofs against the plutonistic and in favor of the crystalline origin of the heavenly bodies. The chapter on these mysterious visitors from the celestial spaces is the longest and certainly one of the most interesting.

His views, especially in regard to the constant metamorphosis of rock-strata, are in fair way of becoming generally accepted—although his name is not as yet mentioned in connection with them.*

Another theory which refers the formation of coal-fields to the deposition of immense masses of sea-weeds and tangs at the bottom of the ocean, has been greatly supported by the discovery that Iodine and Bromine are regularly found in the smoke-black from chimneys where coal is burned.

The book abounds in new and original researches as well as in bold deductions; and even those who do not agree with the author will find in it an almost inexhaustible source of information, and will experience that great delight which the writings of a great thinker always give to an impartial reader.

In conclusion, it may be safely said that MOHR belonged to those whose writings and the results of whose labors will not lose interest as time passes, but will rather be more and more generally appreciated. He has said or written but very little that he was forced to revoke; on the contrary, many of his views have stood against the attack of time and of his adversaries, and many of the latter have been forced to admit that he was right.

Personally, as has been mentioned already, FRIEDRICH MOHR was the most amiable of men, and the Editor of the '*Gaea*,' DR. H. KLEIN, who was a near friend of his during many years, rightly says of him (in a private letter to the writer):

"In every respect MOHR was a man who would be an ornament to any period of Human History."

* LIEBIG'S "Annalen;" Vol. 195, 1879; pp. 133-213.

NOTE.—The rest of this series which the editor of the *Annalen* declined to publish, are those mentioned in the foot-note on p. 203 of No. 17 of this journal.

* *Die Fortschritte der Geologie*, 1874-'75; *p.* 57.

** In a future article this subject will be exhaustively treated, and the position, taken by VOLGER, MOHR and RADENHAUSEN in regard to the nebular hypothesis, as well as that of *Sir* WILLIAM THOMSON POULET, SKROUP, STERRY HUNT and others in regard to the Earth's molten Interior, will be thoroughly treated of and criticized. A letter, also, from the pen of one of America's first astronomers will be quoted, in which this eminent scientist states his views on the subject in a manner which adds the additional weight of his superior authority to the evidence adduced against this Hypothesis. G. W. R.

* Strata of Shales, Mica-Schist, Calcareous Schist and Gneiss not infrequently are so uniformly spread out, in the same locality, that there can be no doubt of their common origin.

CORRESPONDENCE.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

PYROLOGY.

To H. C. Sorby, Esq., F.R.S., LL. D., etc.

My Dear Sir,—As you are the only scientific Englishman of note who seems to have studied blowpipe crystallizations with the view of applying your observations geologically, I will not apologize for addressing you on this interesting subject, and I do so publicly for the following reason, which I think ought to be made public:

The last ten years of my life have been wasted in vainly trying to show that blowpipe chemistry, if studied on the rational basis of ordinary chemical analysis, *i. e.*, employing acids or alkalies and not salts, in the first place, as weapons of attacking substances, will inevitably lead to new discoveries and most interesting results in what scientific men of all branches are agreed to term their common pursuit—the revelation of Nature's secrets,—but my humble efforts have been met, in England, not merely with what Mr. Crookes has called "the conspiracy of silence," but with the most determined, if not underhand, opposition. Indeed, a foreigner considering the circumstances related to him, in which neither my purse, nor time, nor mental efforts, have been spared—hitherto, only to my own disadvantage—said "it appeared more as if I had been trying to commit a felony in England than to advance science there." I readily admit that the three exceptions to this category of eminent scientific enemies constitute a trinity of talent not easily found elsewhere, but then there are only three, if three of the leading men of science in Britain, and I fear therefore, that it was rather unadvised of me to forward my paper on this subject to you for presentation to the British Association, lately assembled at Swansea, and which was returned to you by the Secretary of the Chemical Section (to whom you seem to have forwarded it) as being "Unsuitable."

It will therefore afford you some consolation to learn that this paper, so ignominiously rejected at Swansea, was read before the German Association at Dantzic, on the 23d of September, by one of the chemists on the Swansea Committee, Professor Gilbert Wheeler, of the University of Chicago, United States of America, who had it translated into German for the purpose, and he informed me that one of the learned gentlemen there expressed "his astonishment" that the paper (a very brief one) had not been read in England;—adding, "in *our* country, when anything is objected to in a paper, *that* constitutes the greater reason for reading and discussing it."

So much for personal matters, and now, putting individual injustice or recognition aside, does it not appear to you that the rejection of any contribution, however feeble, towards the advancement of science "by a section of an association originally organized for that purpose by two Scotsmen—Sir David Brewster and Sir Roderick Murchison—shows a lack of what Sir John Herschel terms "that central thread of common sense on which the pearls of analytical research are invariably strung?"

The question seems to me not to be "Has England as many learned professors as Germany or France?" but have the masses of the people—the people, for instance, whom we may see so devoutly thronging the public houses and gin palaces in London on Sunday evening, when anything in the shape of scientific instruction would be considered "a desecration of the Sabbath"—have these poor religious people as much opportunity and possibility, within their little means, afforded them of acquiring practical scientific knowledge (which after all, underlies all art and labor) as the same classes have in Germany, France, or America?

The following little anecdote, among many other similar ones, shows that they have not. The other day, passing a book stall in the West of London, I asked a youth of 19 or 20, in charge, "if he had any books on chemistry?" "Chemistry," said he—"what's that?" I rejoined to this rather startling question—"I suppose you are a pupil of the London School Board?"—to which he replied "yes." I then said "what did they teach you, if they didn't teach chemistry?"—whereupon, to my grave satisfaction, he said "Oh, we learnt all about placental mammals, and vertebrata and all that"—an answer which shows that Mr. Huxley's remonstrances with the London School Board have not been altogether in vain.

With this little illustration of the state of things scientific at our very doors I will conclude this letter, and propose, with your leave, to consider in my next, the subject of your admirable address to the geological section at Swansea, of which you are President.

W. A. Ross, Lieut.-Col., R. A.

CHEMICAL NOTES.

The Sun has a Sensible Inductive Action on the Earth, even when its Magnetic Power is simply equal to that of our Globe. Induction of the Moon by the Earth and Diurnal Lunar Variation of the Terrestrial Compass.—M. Quet has shown that the sun induces the earth in various manners; by its rotation, by the speed of the earth in its orbit, by the rotation of the earth, and by the variations which it experiences in its electric constitution. The electromotive forces due to the three first-mentioned causes are:—The first 14 times greater than the second, and the second 72 times greater than the third.

The Variations of the Coefficient of Expansion of Glass.—J. M. Crafts has summed up, in his former papers, the most important theories on the variation of the fixed points of thermometers, but the variation of the coefficient of expansion of glass, which presents a much more serious inconvenience, has hitherto escaped notice. If this coefficient varies, the interval between two fixed points varies, and the graduation becomes inexact. In thermometers heated for a long time to 355°, the coefficient of expansion decreases, so that whilst the zero-point is raised by t degrees, the point 100° is raised to $100° + t + l$.

Tungstoboric Acid.—According to D. Klein, this acid differs in its constitution from various other borotungstic acids which have been prepared, and is the analogue of the unknown decatungstic acid. It is formed by the union of 9 mols. tungstic acid, 1 mol. dimetaboric hydrate, with elimination of 6 mols. water. Its composition is—

$$9WO_3, B_2O_3, 4H_2O.$$

Products of the Distillation of Colophonium.—Ad. Renard has isolated a carbide, which he names heptene, of the sp. gr. 0.8031 at + 20°. It is without action upon polarized light, and boils at 103° to 106°. He examined its behavior with reagents.

Dilatation and the Compressibility of Gases under Strong Pressures.—E. H. Amagat concludes from his researches that the coefficient of expansion of gases for temperatures above the critical temperature increases with pressure up to a maximum, on passing which it decreases indefinitely. The maximum diminishes for the more elevated temperatures, and finally disappears. For pressures lower than the critical pressure the deviation, which is at first positive at a temperature sufficiently low, becomes null, and then negative as the temperature increases; but, proceeding from a certain negative value, it diminishes indefinitely without changing its sign. For the pressures comprised between the critical pressure and a superior limit, special for each gas, the period during which the deviation is positive is preceded by a period where it is negative, so that the deviation changes its sign twice.

New Results of the Utilization of Solar Heat Obtained at Paris.—M. A. Pifre's improved apparatus enables him to utilize 80 per cent. of the solar heat, thus obtaining, at Paris, 12.12 cal. per minute and per square metre of surface exposed to the sun.

Remarkable Instance of Lightning Ascending Vertically.—A. Trécul perceived, during the storm of the evening of August 19th, lightning ascending perpendicularly behind the trees of the Place Jussieu, apparently from the conductors of the wine magazine.

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The last ten years of my life have been wasted in vainly trying to show that blowpipe chemistry, if studied on the rational basis of ordinary chemical analysis, *i. e.*, employing acids or alkalies and not salts, in the first place, as weapons of attacking substances, will inevitably lead to new discoveries and most interesting results in what scientific men of all branches are agreed to term their common pursuit—the revelation of Nature's secrets,—but my humble efforts have been met, in England, not merely with what Mr. Crookes has called "the conspiracy of silence," but with the most determined, if not underhand, opposition. Indeed, a foreigner considering the circumstances related to him, in which neither my purse, nor time, nor mental efforts, have been spared—hitherto, only to my own disadvantage—said "it appeared more as if I had been trying to commit a felony in England than to advance science there." I readily admit that the three exceptions to this category of eminent scientific enemies constitute a trinity of talent not easily found elsewhere, but then there are only three, if three of the leading men of science in Britain, and I fear therefore, that it was rather unadvised of me to forward my paper on this subject to you for presentation to the British Association, lately assembled at Swansea, and which was returned to you by the Secretary of the Chemical Section (to whom you seem to have forwarded it) as being "Unsuitable."

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So much for personal matters, and now, putting individual injustice or recognition aside, does it not appear to you that the rejection of any contribution, however feeble, towards the advancement of science "by a section of an association originally organized for that purpose by two Scotsmen—Sir David Brewster and Sir Roderick Murchison—shows a lack of what Sir John Herschel terms "that central thread of common sense on which the pearls of analytical research are invariably strung?"

The question seems to me not to be "Has England as many learned professors as Germany or France?" but have the masses of the people—the people, for instance, whom we may see so devoutly thronging the public houses and gin palaces in London on Sunday evening, when anything in the shape of scientific instruction would be considered "a desecration of the Sabbath"—have these poor religious people as much opportunity and possibility, within their little means, afforded them of acquiring practical scientific knowledge (which after all, underlies all art and labor) as the same classes have in Germany, France, or America?

The following little anecdote, among many other similar ones, shows that they have not. The other day, passing a book stall in the West of London, I asked a youth of 19 or 20, in charge, "if he had any books on chemistry?" "Chemistry," said he—"what's that?" I rejoined to this rather startling question—"I suppose you are a pupil of the London School Board?"—to which he replied "yes." I then said "what did they teach you, if they didn't teach chemistry?"—whereupon, to my grave satisfaction, he said "Oh, we learnt all about placental mammals, and vertebrata and all that"—an answer which shows that Mr. Huxley's remonstrances with the London School Board have not been altogether in vain.

With this little illustration of the state of things scientific at our very doors I will conclude this letter, and propose, with your leave, to consider in my next, the subject of your admirable address to the geological section at Swansea, of which you are President.

W. A. Ross, Lieut.-Col., R. A.

CHEMICAL NOTES.

The Sun has a Sensible Inductive Action on the Earth, even when its Magnetic Power is simply equal to that of our Globe. Induction of the Moon by the Earth and Diurnal Lunar Variation of the Terrestrial Compass.—M. Quet has shown that the sun induces the earth in various manners; by its rotation, by the speed of the earth in its orbit, by the rotation of the earth, and by the variations which it experiences in its electric constitution. The electromotive forces due to the three first-mentioned causes are:—The first 14 times greater than the second, and the second 72 times greater than the third.

The Variations of the Coefficient of Expansion of Glass.—J. M. Crafts has summed up, in his former papers, the most important theories on the variation of the fixed points of thermometers, but the variation of the coefficient of expansion of glass, which presents a much more serious inconvenience, has hitherto escaped notice. If this coefficient varies, the interval between two fixed points varies, and the graduation becomes inexact. In thermometers heated for a long time to 355°, the coefficient of expansion decreases, so that whilst the zero-point is raised by t degrees, the point 100° is raised to $100° + t + l$.

Tungstoboric Acid.—According to D. Klein, this acid differs in its constitution from various other borotungstic acids which have been prepared, and is the analogue of the unknown decatungstic acid. It is formed by the union of 9 mols. tungstic acid, 1 mol. dimetaboric hydrate, with elimination of 6 mols. water. Its composition is—

$$9WO_3, B_2O_3, 4H_2O.$$

Products of the Distillation of Colophonium.—Ad. Renard has isolated a carbide, which he names heptene, of the sp. gr. 0.8031 at + 20°. It is without action upon polarized light, and boils at 103° to 106°. He examined its behavior with reagents.

Dilatation and the Compressibility of Gases under Strong Pressures.—E. H. Amagat concludes from his researches that the coefficient of expansion of gases for temperatures above the critical temperature increases with pressure up to a maximum, on passing which it decreases indefinitely. The maximum diminishes for the more elevated temperatures, and finally disappears. For pressures lower than the critical pressure the deviation, which is at first positive at a temperature sufficiently low, becomes null, and then negative as the temperature increases; but, proceeding from a certain negative value, it diminishes indefinitely without changing its sign. For the pressures comprised between the critical pressure and a superior limit, special for each gas, the period during which the deviation is positive is preceded by a period where it is negative, so that the deviation changes its sign twice.

New Results of the Utilization of Solar Heat Obtained at Paris.—M. A. Pifre's improved apparatus enables him to utilize 80 per cent. of the solar heat, thus obtaining, at Paris, 12.12 cal. per minute and per square metre of surface exposed to the sun.

Remarkable Instance of Lightning Ascending Vertically.—A. Trécul perceived, during the storm of the evening of August 19th, lightning ascending perpendicularly behind the trees of the Place Jussieu, apparently from the conductors of the wine magazine.

BOOKS RECEIVED.

AMERICAN SCIENCE SERIES—BOTANY—FOR HIGH SCHOOLS AND COLLEGES. By Charles E. BESSEY, M.Sc. Ph.D., Professor of Botany in the Iowa Agricultural College. Henry Holt & Company, New York, Large 12mo. 1880.

Circumstances, ever varied in their nature, daily remind us of the progress of science, but the production of a really valuable manual devoted to some special line of research not only gives direct evidence of progress already achieved, but hopefully suggests future advancement. For these reasons, we welcome a new manual of botany, written by Professor Charles E. Bessey, of the Iowa Agricultural College, which presents many advantages over previous publications having the same object in view, and must prove one of the most valuable aids to a true knowledge of the vegetable kingdom which the advanced student can possess.

Although modestly styled by the author "*An Introduction to the Study of Plants*," the work appears to leave little unexplained which is requisite for a comprehension of the anatomy and physiology of the vegetable. It is not claimed that the material is new, but the original arrangement of the matter to secure a more logical presentation of the subject, is apparent throughout the work.

Professor Bessey directs attention to two innovations which he has made, consisting of the "recognition of seven quite well marked kinds of tissue," and that of "raising the Protophyta, Zygosporeæ, Oosporeæ and Carposporeæ to the dignity of Primary Divisions of the Vegetable Kingdom, co-ordinate with the Bryophyta, Pteridophyta and Phanerogamia," in the hope that they may serve to give a clearer and more accurate notion of the structure of plants.

To those unacquainted with the German language, and to whom, therefore, the works of the German botanists are as sealed books, the present manual will prove particularly valuable, as free use has been made of the works of Sachs, DeBary, Hofmeister, Strasburger, Nägeli, Schwendener and others, while many of the cuts in Sachs' "Lehrbuch" have been reproduced.

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Professor Bessey divides his manual into two Parts, the First of which is based on Sachs' "Lehrbuch," the general plan of which is closely followed. The first chapter appropriately opens with a description of the "active and vital" principles of all vegetable organisms, "*Protoplasm*." Following the plant cell, is discussed the cell wall, the formation of new cells the product of the cell, tissues, inter-cellular spaces, and secretion reservoirs, and so on until the plant body is gradually built up. The last three chapters of this portion of the work relate to the chemical constituents of plants, the chemical processes in the plant, and the relations of plants to external agents.

The student having thus become familiarized with the anatomy and general structure of plants, the author, in Part Two, presents his plan of classification, which, as we have stated, is based on that made use of by Sachs for the lower orders of plants, while that for the higher plants conforms more nearly to the system of classification recognized in this country and in England. Professor Bessey divides the vegetable kingdom into six divisions, as follows:

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This is a departure from the classification which has so long been followed in the English works on botany, the familiar groups of Algæ and Fungi are not recognized, the terms being retained only when general reference is made to the Chlorophyll-bearing and the Chlorophyll-free Thallophytes, Professor Bessey stating that, under his arrangement, the term Algæ implies a Thallophyte which contains Chlorophyll, and that by a Fungus is understood one which is Saprophytic or Parasitic in habit, and which is, in consequence, free from Chlorophyll.

In the classification of the Diatomaceæ, that proposed by Professor H. L. Smith, one of the best authorities on the subject, has been wisely followed, which divides the order into three tribes, each containing several families.

As the classification of the Diatomaceæ is as yet largely artificial, we presume the one adopted by Professor Smith is provisional.

We have probably indicated sufficiently in this outline the leading characteristics of this last, and, in our opinion, the best Manual of Botany. Its merits are apparent throughout the work, and it is evident that Professor Bessey has spared no pains to render his work perfect and worthy of the great subject treated.

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NOTES AND QUERIES.

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$Ba_3\,(PO_4)_2 + Ba\,I_2 + 2\,Co_2 + 2H_2O.$

(2.) $Ba\,I_2 + K_2\,So_4 = 2KI + Ba\,So_4.$

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